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Grand challenges in the research on soil processes

Philippe C. Baveye *

Laboratory of Soil and Water Engineering, Department of Civil and Environmental Engineering, Jonsson Engineering Center, Rensselaer Polytechnic Institute, Troy, NY, USA
Correspondence: baveye.rpi@gmail.com

Edited by:
Jacques Berthelin, Université de Lorraine, France

Reviewed by:
Hans-Joerg Vogel, Helmholtz Centre for Environmental Research - UFZ, Germany

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The exercise of identifying the key challenges faced by a discipline at a given moment can be rewarding. History suggests that soil science has tended to leap forward most significantly when a momentous event or significant societal pressure encouraged researchers to focus their attention on a single challenge, or at most on a few of them. For example, in the middle of the eighteenth century, following a period of pronounced and worrisome decline of crop yields in Europe, the processes that controlled soil fertility emerged as a pressing area of research. A number of science academies in several European countries devoted considerable attention to the topic (e.g., Baveye, 2013), which eventually paved the way for the mid-nineteenth century research on mineral fertilizers, and resulted in sizeable increases in agricultural productivity. Again, in the 1930s, public concern over the health and environmental impacts of the major dust storms associated with the "Dust Bowl" in the United States caused a significant research effort and major soil conservation programs to be launched (e.g., Baveye et al., 2011; Sylvester and Rupley, 2012).

In some disciplines, researchers have been able to identify key challenges through collective reflection. In theoretical physics, for example, there is widespread consensus that the development of a satisfactory Grand Unified Theory is a key challenge, and legions of researchers are trying to tackle it, from a wide range of perspectives. In relation to soils, however, it has proven difficult for researchers to make such a consensus emerge. In the rare occasions where the soil science community has attempted to identify key challenges, the results have been underwhelming. For example, 5 years ago, the Soil Science Society of America assigned to a "Grand Challenge Workgroup" the task of identifying the challenges in the discipline of soil science. The group ended up with a long shopping list of short-, medium-, and long-term priorities related, among other things, to human and ecosystem health, water treatment and water quality, food and energy security, and climate change (Soil Science Society of America, 2011). Similarly, a recent survey of leading researchers, policy experts, and organizational leaders (Adewopo et al., 2014) produced a lengthy enumeration of 25 "top-ranked" priority research questions, covering a very broad range of issues. Clearly, in the soil science community at least, key challenges cannot be identified by committees of experts, as each expert appears to insist on having his or her pet research topic included in the final list.

A better way to zero in on key questions about soils that scientists should try to address in priority, might be to review the major societal concerns of the moment, and determine how soils enter into the picture in each case. In this respect, many analysts agree on the fact that humanity is currently confronted with two major long-term threats to its sustainability and even survival. The first relates to food security, and the second to ecological threats brought about by global climate change. In both cases, soils constitute an important component of the equation, and increasingly pressing demands are made to soil scientists to either come up with solutions, or to at least anticipate future trends.

In terms of food security, it is largely unclear at the moment how one will be able to produce the food needed to sustain the 8 billion people that will inhabit the earth by 2025, climbing to 9 billion by 2050. Estimates are that, by the mid-twenty first century, food production will have to increase by 100%, if not more, relative to current levels. For various reasons (Sposito, 2013), it is no longer possible to think of increasing the area of land under cultivation worldwide. Neither is it an option to increase the amount of ground- or surface water used for irrigation. In both cases, any further increase would likely result in severe ecological damages, with dire long-term consequences. Phosphate fertilizers may also become limiting, mostly for geopolitical reasons (in part because 75 to 80% of current P reserves are concentrated in a single region of the world, in Morocco and Western Sahara).

Given these different constraints, one could try to modify agricultural practices so that the soil layer colonized by crop roots be able to retain more water and more nutrients than is currently the case, thereby enabling crop yields to increase. Some authors have suggested that the most direct way to obtain the desired effect might be to stimulate the release by plants of various types of exudates, which would trigger microbial feedback processes in the rhizosphere, and in turn would contribute to making water and different nutrients (including P), held by soils, more available to plants. However, this may be easier said than done. Indeed, many obstacles lie at the moment on the path ahead. Aside from the inherent complexity and plasticity of root architecture, attempts to direct plant-soil feedback mechanisms in one direction or another are often hindered by the high biodiversity of soil microorganisms, only about 1.5% of which have been characterized at this stage (e.g., Stein and Nicol, 2011). Research is needed to elucidate how specific root exudates influence microbial processes in the rhizosphere, and what consequences they may have on a range of soil physical and chemical properties, for example in terms of nutrient movement and soil structural stability. With a myriad of interactions possible among the thousands of microbial species present in soils and between those species and plant roots, the intrinsic complexity of the system is enormous, and risks are high that attempts to stimulate a given plant-soil feedback might result simultaneously in the proliferation of pathogens or in problematic shifts in soil micro- and macrofaunal populations.

On a different front, soils are also expected to play a crucial role in global climate change, because of the large amount of carbon that they contain. Current estimates of global soil C are slightly over 4000 Pg C, which is more than five and a half times the amount of carbon currently in the atmosphere or, put differently, is equivalent to about 400 times the amount of C released yearly to the atmosphere by fossil fuel consumption or cement manufacture (Baveye et al., 2011). Therefore, even a small drop, of the order of a percent, of the amount of carbon contained in soils, due to a rise in ambient temperature and a resulting stimulation of microbial metabolism, could lead in the long run to a very noticeable increase in atmospheric C and a devastatingly positive feedback to climate change (Baveye, 2007; Reichstein and Beer, 2008). A sizeable research effort has been devoted to this issue in the last three decades, resulting in thousands of research publications and reports. Nevertheless, this sustained work has met with limited success to date. Over the years, many researchers have pointed out how remarkably difficult it is to obtain conclusive evidence on most aspects of the issue (e.g., Kirschbaum, 1995, 2006; Davidson and Janssens, 2006; Billings and Ballantyne, 2013). Depending on the experimental approach used and the particular feature of organic matter decomposition being investigated, it is possible to find reports demonstrating that increases in temperature have a strong positive effect (Fang et al., 2005), no noticeable effect (Giardina and Ryan, 2000), or even a negative effect on decomposition in the long-term (Dalias et al., 2001). Faced with this confusion, Wu et al. (2011) observe that "the general responses of C stocks in terrestrial ecosystems to changes in environmental conditions, especially temperature and precipitation, and their combined effects, remain unclear". Similarly, in a recent article, Hamdi et al. (2013) conclude from an exhaustive meta-analysis of the literature that the large variability of observed temperature sensitivities of soil carbon dynamics is partly related to the methods used, to the time of incubation, to temperature ranges considered, and to differences in initial soil carbon content, but otherwise is still "largely unexplained."

An increasingly accepted explanation of the lack of progress regarding the processes that control the fate of carbon in soils is that traditional macroscopic measurements (e.g., of organic matter content, microbial population density, or genetic diversity) are unable to capture, even qualitatively, the key features of microbial activity in soils, and that crucial aspects of the intricate conformation of microbial micro-environments at sub-macroscopic scales are being missed (Baveye et al., 2011; Baveye and Laba, 2014). Therefore, to address the challenges facing soils, we need urgently to (1) be able to characterize the spatial heterogeneity of soil properties at the micrometric scale, directly relevant to microorganisms, (2) try to understand experimentally how microorganisms relate to their physical environment at that scale, and (3) develop models that encapsulate this information and make predictions of future trends possible. The very same needs arise in terms of the processes that underlie plant-soil feedback mechanisms.

This assessment is not new. More than 50 years ago already, various authors (e.g., Alexander, 1964) argued that progress in many areas of soil microbiology required that quantitative observations be made at the scale of microorganisms. Unfortunately, researchers could do very little along these lines at the time, because of a lack of appropriate technologies. For many years, the only equipment that was of any help in this respect was the scanning electron microscope, which could be used to visualize microbial micro-environments in soils (Foster, 1988), but unfortunately delivered no quantitative or spatial data. This situation has changed dramatically in the last few years. Significant technological advances have provided soil researchers with routine access to X-ray computed tomography systems, which, as methodological roadblocks are being resolved (e.g., Elliot and Heck, 2007; Iassonov et al., 2009; Baveye et al., 2010; Iassonov and Tuller, 2010; Hapca et al., 2013; Houston et al., 2013a,b), increasingly provide reliable information about the geometry of pores and solids in soils at resolutions as small as 0.5 μm. Concomitant progress in near-edge X-ray spectromicroscopy (NEXAFS), synchrotron X-ray absorption spectroscopy, and synchrotron-based micro-fluorescence spectroscopy of thin sections of soils has led to observations of sharp spatial heterogeneity in the chemical make-up of soil organic matter over minute distances, respectively of the order of nanometers to micrometers (Schumacher et al., 2005; Wan et al., 2007), and in the accumulation of trace metals (Jacobson et al., 2007; Strawn and Baker, 2008, 2009; Prietzel et al., 2010; Thieme et al., 2010). Significant advances related to biological markers now allow specific bacteria to be identified in soils and their spatial distribution at micrometric scales to be determined in thin sections (Eickhorst and Tippkötter, 2008a,b), and this information can be translated into 3-dimensional distributions using recently developed statistical algorithms (Hapca et al., 2011). In addition, very efficient modeling tools, like the Lattice-Boltzmann model, allow the description of transport and physico-chemical processes occurring in soil pores at scales directly relevant to microorganisms (e.g., Vogel et al., 2005; Falconer et al., 2012; Genty and Pot, 2013), whereas individual-based models, also developing rapidly (Gras et al., 2010), can describe the dynamics of small groups of microorganisms inhabiting the pore space (e.g., Garnier et al., 2008).

Beyond a satisfactory understanding of how the microscale spatial heterogeneity of soils influences biological, chemical, and physical processes, as well as their emergence or manifestation at the macroscopic scale, a key objective of the research in this area will have to be to provide some guidance regarding the type of macroscopic measurements one should perform on soils. While it is clear that many measurements currently carried out, and which end up being displayed in the numerous soil maps that are produced, do not provide a whole lot of useful information about soil processes (Baveye, 2009; Baveye and Laba, 2014), it is far less obvious at the moment which practical measurements would make sense. Therefore, a significant research effort should be devoted to "upscaling", where one not only analyzes how microscopic features and processes translate into "emergent" properties at the macroscopic scale, but one also tries to identify the measurable features of soil systems that control their macroscopic behavior. Once these questions will be resolved, the next step in the research should consist of finding a path from the typically local perspective unraveled by measuring instruments, to the broader scales of catchments, countries, or even continents, where soil-related food security or global climate change issues ultimately need to be addressed and resolved. It may be that this further change of scale will require the use of spatial statistics, and in particular of geostatistics, in a manner akin to that of past research on the spatial "variability" of soils, or it may require an entirely new approach and a different mathematical toolbox (Baveye and Laba, 2014).

This rapid overview of the foremost challenges facing the research on soils would not be complete without mentioning what is perhaps the most daunting societal demand made to the soil science community at the moment. Some 20 years ago, a number of ecologists and environmental economists took advantage of a favorable political climate in the US to promote the idea that the best way to preserve nature was to associate a monetary value to the numerous so-called "ecosystem services" it renders to human populations (Baveye et al., 2013; Baveye, 2014). Whereas the ecosystem service idea has been espoused enthusiastically by soil scientists (Dominati et al., 2010; Robinson et al., 2013), very little progress has been made to date on the monetization of soil services. This may be due to the fact that it is not straightforward to assign a price to features or processes one does not understand satisfactorily, or the slow progress might be related more to uncertainty and lack of trust about what financiers might do with prices associated to soil services. Nevertheless, significant pressure is currently exerted on soil scientists by national governments and international agencies to engage actively with the ecosystem services framework. The challenge for soil scientists is either to find ways to monetize soil services meaningfully, or to demonstrate convincingly (and relatively rapidly) that there are alternative paths that can be followed to preserve soils without necessarily putting price tags on their services. In both cases, soil scientists will have to learn to collaborate closely with economists and sociologists, which in a number of ways will be an entirely new experience for many of them.

Actually, this need to work with researchers from other intellectual horizons is becoming a constant in soil science, and is representing yet another significant challenge, in and of itself. Indeed, as with the work on the ecosystem services of soils, investigations of both plant-soil feedbacks and the fate of carbon in soil, by their very nature, need imperatively to be interdisciplinary, i.e., involve researchers initially from different disciplines, willing to work together, in very close association, toward a common goal. For interdisciplinarity to be more than a buzzword, participants should all agree, at the onset of any new research program that is launched, on an initial step where each person learns about the disciplines of the others, a common language is developed, and if needed, entirely new methodologies are elaborated. For example, microbiologists need to understand, at a very fundamental level, what plant scientists, soil physicists, (bio)chemists, or modelers are doing, and vice-versa. Soil scientists need to understand that what economists refer to when they talk about "economic theory" really does not have much in common with the theories we are used to in science, and economists in reverse have to grasp how scientists approach things and why they may be unwilling to follow some paths... Reaching such a mutual understanding takes time, during which actual research breakthroughs may be few and far between. Perhaps because this first learning step is seldom taken seriously enough, experience shows that when the actual research takes place, slippages back to mono-disciplinary or multi-disciplinary modus operandi are frequent (Baveye et al., 2014).

To ensure that significant progress take place in the interdisciplinary endeavors that are needed to meet soil science challenges, a further roadblock has to be overcome. Even though many funding agencies, in the US, in Europe, or elsewhere, claim to be very supportive of interdisciplinary research, virtually none has structures in place that are truly adapted to this type of activity. Interdisciplinary projects often need to involve sizeable groups of investigators working in very close proximity, which means that the budget allocations have to be substantially larger than they are at the moment. Soil scientists should try consistently to educate funding bodies and policy makers of the need for funding structures to be better adapted to the research carried out on soils. Certainly, in other areas, some of our colleagues have been successful in convincing governments to invest huge financial resources into the construction of costly particle accelerators or into the launching of satellites to identify exoplanets many light years away from earth (Baveye et al., 2011). There is no real reason why we could not be successful as well.

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